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EXPERIMENTS

ON

AIR.

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EXPERIMENTS ON AIR.

THE following experiments were made principally with a view to find out the cauſe of the diminution which common air is well known to ſuffer by all the various ways in which it is phlogiſticated, and to diſcover what becomes of the air thus loſt or condenſed; and as they ſeem not only to determine this point, but alſo to throw great light on the conſtitution and manner of production of dephlogiſticated air, I hope they may be not unworthy the acceptance of this ſociety.

Many gentlemen have ſuppoſed that fixed air is either generated or ſeparated from atmoſpheric air by phlogiſtication, and that the obſerved diminution is owing to this cauſe; my firſt experiments therefore were made in order to aſcertain whether any fixed air is really produced thereby. Now, it muſt be obſerved, that as all animal and vegetable ſubſtances contain fixed air, and yield it by burning, diſtillation, or putrefaction, nothing can be concluded from experiments in which the air is phlogiſticated by them. The only methods I know, which are not liable to objection, are by the calcination of metals, the burning of ſulphur or phoſphorus, the mixture of nitrous air, and the exploſion of inflammable air. Perhaps it may be ſuppoſed, that I ought to add to theſe the electric ſpark; but I think it much moſt likely, that the phlogiſtication of the air, and production of fixed air, in this proceſs, is owing to the burning of ſome inflammable matter in the apparatus. When the ſpark is taken from a ſolution of tournſol, the burning of the tournſol may produce this effect; when it is taken from lime-water, the burning of ſome foulneſs adhering to the tube, or perhaps of ſome inflammable matter contained in the lime, may have the ſame effect; and when quickſilver or metallic knobs are uſed, the calcination of them may contribute to the phlogiſtication of the air, though not to the production of fixed air.

There is no reaſon to think that any fixed air is produced by the firſt method of phlogiſtication. Dr. PRIESTLEY never found lime-water to become turbid by the calcination of metals over it*: Mr. LAVOISIER alſo found only a very ſlight and ſcarce perceptible turbid appearance, without any precipitation, to take place when lime-water was ſhaken in a glaſs veſſel full of the air in which lead had been calcined; and even this ſmall diminution of tranſparency in the lime-water might very likely ariſe, not from fixed air, but only from its being fouled by particles of the calcined metal, which we are told adhered in ſome places to the glaſs. This want of turbidity has been attributed to the fixed air uniting to the metallic calx, in preference to the lime; but there is no reaſon for ſuppoſing that the calx contained any fixed air; for I do not know that any one has extracted it from calces prepared in this manner; and though moſt metallic calces prepared over the fire, or by long expoſure to the atmoſphere, where they are in contact with fixed air, contain that ſubſtance, it by no means follows that they muſt do ſo when prepared by methods in which they are not in contact with it.

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* Experiments on Air, vol. I. p. 137.

Dr. PRIESTLEY alſo obſerved, that quickſilver, fouled by the addition of lead or tin, depoſits a powder by agitation and expoſure to the air, which conſiſts in great meaſure of the calx of the imperfect metal. He found too ſome powder of this kind to contain fixed air *; but it is by no means clear that this air was produced by the phlogiſtication of the air in which the quickſilver was ſhaken; as the powder was not prepared on purpoſe, but was procured from quickſilver fouled by having been uſed in various experiments, and may therefore have contained other impurities beſides the metallic calces.

I never heard of any fixed air being produced by the burning of ſulphur or phoſphorus; but it has been aſſerted, and commonly believed, that lime water is rendered cloudy by a mixture of common and nitrous air; which, if true, would be a convincing proof that on mixing thoſe two ſubſtances ſome fixed air is either generated or ſeparated; I therefore examined this carefully. Now it muſt be obſerved, that as common air uſually contains a little fixed air, which is no eſſential part of it, but is eaſily ſeparated by lime water; and as nitrous air may alſo contain fixed air, either if the metal from which it is procured be ruſty, or if the water of the veſſel in which it is caught contain calcareous earth, ſuſpended by fixed air, as moſt waters do, it is proper firſt to free both airs from it by previouſly waſhing them with lime water †. Now I found, by repeated

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* Exper. in Nat. Phil. vol. I. p. 144.

† Though fixed air is abſorbed in conſiderable quantity by water, as I ſhewed in Phil. Tranſ. vol. LVI. yet it is not eaſy to deprive common air of all the fixed air

ed experiments, that if the lime water was clean, and the two airs were previouſly waſhed with that ſubſtance, not the leaſt cloud was produced, either immediately on mixing them, or on ſuffering them to ſtand upwards of an hour, though it appeared by the thick clouds which were produced in the lime water, by breathing through it after the experiment was finiſhed, that it was more than ſufficient to ſaturate the acid formed by the decompoſition of the nitrous air, and conſequently that if any fixed air had been produced, it muſt have become viſible. Once indeed I found a ſmall cloud to be formed on the ſurface, after the mixture had ſtood a few minutes. In this experiment the lime water was not quite clean; but whether the cloud was owing to this circumſtance, or to the air's having not been properly waſhed, I cannot pretend to ſay.

Neither does any fixed air ſeem to be produced by the exploſion of the inflammable air obtained from metals, with either common or dephlogiſticated air. This I tried by putting a little lime-water into a glaſs globe fitted with a braſs cock, ſo as to make it air tight, and an apparatus for firing air by electricity. This globe was exhauſted by an air-pump, and the two airs, which had been previouſly waſhed with lime-water, let in, and ſuffered to remain ſome time, to ſhew whether they would affect the lime-water, and then fired by electricity. The event was, that not the leaſt cloud was produced in the lime-water, when the inflammable air was mixed with common air, and

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air contained in it by means of water. On ſhaking a mixture of ten parts of common air, and one of fixed air, with more than an equal bulk of diſtilled water, not more than half of the fixed air was abſorbed, and on transferring the air into freſh diſtilled water only half the remainder was abſorbed, as appeared by the diminution which it ſtill ſuffered on adding lime water.

only a very ſlight one, or rather diminution of tranſparency, when it was combined with dephlogiſticated air. This, however, ſeemed not to be produced by fixed air; as it appeared inſtantly after the exploſion, and did not increaſe on ſtanding, and was ſpread uniformly through the liquor; whereas if it had been owing to fixed air, it would have taken up ſome ſhort time before it appeared, and would have begun firſt at the ſurface, as was the caſe in the abovementioned experiment with nitrous air. What it was really owing to I cannot pretend to ſay; but if it did proceed from fixed air it would ſhew that only an exceſſively minute quantity was produced *. On the whole, though it is not improbable that fixed air may be generated in ſome chymical proceſſes, yet it ſeems certain that it is not the general effect of phlogiſticating air, and that the diminution of common air is by no means owing to the generation or ſeparation of fixed air from it.

As there ſeemed great reaſon to think, from Dr. Prieſtley's experiments, that the nitrous and vitriolic acids were convertible into dephlogiſticated air, I tried whether the dephlogiſticated part of common air might not, by phlogiſtication, be changed into nitrous or vitriolic acid. For this purpoſe I impregnated ſome milk of lime with the fumes of burning ſulphur, by putting a little of it into a large glaſs receiver, and burning ſulphur therein, taking care to keep the mouth of the receiver ſtopt till the fumes were all abſorbed; after which the air of the receiver was changed, and more ſulphur burnt in it as before, and the proceſs repeated till 122 grains of ſulphur were conſumed. The milk of lime was then filtered and evaporated, but it yielded no nitrous ſalt, nor any other ſubſtance except ſelenite; ſo that no ſenſible quantity of the air was changed

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* Dr. Priestley alſo found no fixed air to be produced by the exploſion of inflammable and common air. Vol. V. p. 124.

into nitrous acid. It muſt be obſerved, that as the vitriolic acid produced by the burning ſulphur is changed by its union with the lime into ſelenite, which is very little ſoluble in water, a very ſmall quantity of nitrous ſalt, or any other ſubſtance which is ſoluble in water, would have been perceived.

I alſo tried whether any nitrous acid was produced by phlogiſticating common air with liver of ſulphur; for this purpoſe I made a ſolution of flowers of ſulphur by boiling it with lime, and put a little of it into a large receiver, and ſhook it frequently, changing now and then the air, till the yellow colour of the ſolution was quite gone; a ſign that all the ſulphur was, by the loſs of its phlogiſton, turned into vitriolic acid, and united to the lime, or precipitated; the liquor was then filtered and evaporated, but it yielded not the leaſt nitrous ſalt.

The experiment was repeated in nearly the ſame manner with dephlogiſticated air procured from red precipitate; but not the leaſt nitrous acid was obtained.

It is well known that common ſelenite is very little ſoluble in water; whereas that procured in the two laſt experiments was very ſoluble, and even cryſtallized readily, and was intenſely bitter; this however appeared to be owing merely to the acid with which it was formed being very much phlogiſticated; for on evaporating it to dryneſs, and expoſing it to the air for a few days, it became much leſs ſoluble, ſo that on adding water to it not much diſſolved; and by repeating this proceſs once or twice, it ſeemed to become not more ſoluble than ſelenite made in the common manner.

This ſolubility of the ſelenite cauſed ſome trouble in trying the experiment; for while it continued much ſoluble it would have been impoſſible to have diſtinguiſhed a ſmall mixture of nitrous ſalt; but by the abovementioned proceſs I was able to diſtinguiſh as ſmall a proportion as if the ſelenite had been originally no more ſoluble than uſual.

The nature of the neutral ſalts made with the phlogiſticated vitriolic and nitrous acids has not been much examined by the chymiſts, though it ſeems well worth their attention; and it is likely that many beſides the foregoing may differ remarkably from thoſe made with the ſame acids in their common ſtate. Nitre formed with the phlogiſticated nitrous acid has been found to differ conſiderably from common nitre, as well as Sal Polychreſt from vitriolated tartar.

In order to try whether any vitriolic acid was produced by the phlogiſtication of air, I impregnated fifty ounces of diſtilled water with the fumes produced on mixing fifty-two ounce meaſures of nitrous air with a quantity of common air ſufficient to decompound it. This was done by filling a bottle with ſome of this water, and inverting it into a baſon of the ſame, and then, by a ſyphon, letting in as much nitrous air as filled it half-full; after which common air was added ſlowly by the ſame ſyphon, till all the nitrous air was decompounded. When this was done, the diſtilled water was further impregnated in the ſame manner till the whole of the abovementioned quantity of nitrous air was employed. This impregnated water, which was very ſenſibly acid to the taſte, was diſtilled in a glaſs retort. The firſt runnings were very acid, and ſmelt pungent, being nitrous acid much phlogiſticated; what came next had no ſenſible taſte or ſmell; but the laſt runnings were very acid, and conſiſted of nitrous acid not phlogiſticated. Scarce any ſediment was left behind. Theſe different parcels of diſtilled liquor were then exactly ſaturated with ſalt of tartar, and evaporated; they yielded 87½ grains of nitre, which, as far as I could perceive, was unmixed with vitriolated tartar or any other ſubſtance, and conſequently no ſenſible quantity of the common air with which the nitrous air was mixed was turned into vitriolic acid.

It appears, from this experiment, that nitrous air contains as much acid as 2¾ times its weight of ſaltpetre; for fifty-two ounce meaſures of nitrous air weigh 32 grains, and, as was before ſaid, yield as much acid as is contained in 87½ grains of ſaltpetre; ſo that the acid in nitrous air is in a remarkably concentrated ſtate, and I believe more than 1½ times as much ſo as the ſtrongeſt ſpirit of nitre ever prepared.

Having now mentioned the unſucceſsful attempts I made to find out what becomes of the air loſt by phlogiſtication, I proceed to ſome experiments, which ſerve really to explain the matter.

In Dr. PRIESTLEY's laſt volume of experiments is related an experiment of Mr. WARLTIRE's, in which it is ſaid that, on firing a mixture of common and inflammable air by electricity in a cloſe copper veſſel holding about three pints, a loſs of weight was always perceived, on an average about two grains, though the veſſel was ſtopped in ſuch a manner that no air could eſcape by the exploſion. It is alſo related, that on repeating the experiment in glaſs veſſels, the inſide of the glaſs, though clean and dry before, immediately became dewy; which confirmed an opinion he had long entertained, that common air depoſits its moiſture by phlogiſtication. As the latter experiment ſeemed likely to throw great light on the ſubject I had in view, I thought it well worth examining more cloſely. The firſt experiment alſo, if there was no miſtake in it, would be very extraordinary and curious; but it did not ſucceed with me; for though the veſſel I uſed held more than Mr. WARLTIRE's, namely, 24,000 grains of water, and though the experiment was repeated ſeveral times with different proportions of common and inflammable air, I could never perceive a loſs of weight of more than one-fifth of a grain, and commonly none at all. It muſt be obſerved, however, that though there were ſome of the experiments in which it ſeemed to diminiſh a little in weight, there were none in which it increaſed *.

In all the experiments, the inſide of the glaſs globe became dewy, as obſerved by Mr. WARLTIRE; but not the leaſt ſooty matter could be perceived. Care was taken in all of them to find how much the air was diminiſhed by the exploſion, and to obſerve its teſt. The reſult is as follows: the bulk of the inflammable air being expreſſed in decimals of the common air,

Common air.	Inflammable air.	Diminution.	Air remaining after the exploſion.	Teſt of this air in firſt method	Standard.
1	1,241	,686	1,555	,055	,0
	1,055	,642	1,413	,063	,0
	,706	,647	1,059	,066	,0
	,423	,612	,811	,097	,03
	,331	,476	,855	,330	,27
	,206	,294	,912	,048	,58

In theſe experiments the inflammable air was procured from zinc, as it was in all my experiments, except where otherwiſe expreſſed: but I made two more experiments, to try whether there was any difference between the air from zinc and that from iron, the quantity of inflammable air being the ſame in both, namely, 0,331 of the common; but I could not find any difference to be depended on between the two kinds of air,

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* Dr. PRIESTLEY, I am informed, has ſince found the experiment not to ſucceed.

either in the diminution which they ſuffered by the exploſion, or the teſt of the burnt air.

From the fourth experiment it appears, that 423 meaſures of inflammable air are nearly ſufficient to completely phlogiſticate 1000 of common air; and that the bulk of the air remaining after the exploſion is then very little more than four-fifths of the common air employed; ſo that as common air cannot be reduced to a much leſs bulk than that by any method of phlogiſtication, we may ſafely conclude, that when they are mixed in this proportion, and exploded, almoſt all the inflammable air, and about one-fifth part of the common air, loſe their elaſticity, and are condenſed into the dew which lines the glaſs.

The better to examine the nature of this dew, 500000 grain meaſures of inflammable air were burnt with about 2½ times that quantity of common air, and the burnt air made to paſs through a glaſs cylinder eight feet long and three-quarters of an inch in diameter, in order to depoſit the dew. The two airs were conveyed ſlowly into this cylinder by ſeparate copper pipes, paſſing through a braſs plate which ſtopped up the end of the cylinder; and as neither inflammable nor common air can burn by themſelves, there was no danger of the flame ſpreading into the magazines from which they were conveyed. Each of theſe magazines conſiſted of a large tin veſſel, inverted into another veſſel juſt big enough to receive it. The inner veſſel communicated with the copper pipe, and the air was forced out of it by pouring water into the outer veſſel; and in order that the quantity of common air expelled ſhould be 2½ times that of the inflammable, the water was let into the outer veſſels by two holes in the bottom of the ſame tin pan, the hole which conveyed the water into that veſſel in which the common air was confined being 2½ times as big as the other.

In trying the experiment, the magazines being firſt filled with their reſpective airs, the glaſs cylinder was taken off, and water let, by the two holes, into the outer veſſels, till the airs began to iſſue from the ends of the copper pipes; they were then ſet on fire by a candle, and the cylinder put on again in its place. By this means upwards of 135 grains of water were condenſed in the cylinder, which had no taſte nor ſmell, and which left no ſenſible ſediment when evaporated to dryneſs; neither did it yield any pungent ſmell during the evaporation; in ſhort, it ſeemed pure water.

In my firſt experiment, the cylinder near that part where the air was fired was a little tinged with ſooty matter, but very ſlightly ſo; and that little ſeemed to proceed from the putty with which the apparatus was luted, and which was heated by the flame; for in another experiment, in which it was contrived ſo that the luting ſhould not be much heated, ſcarce any ſooty tinge could be perceived.

By the experiments with the globe it appeared, that when inflammable and common air are exploded in a proper proportion, almoſt all the inflammable air, and near one-fifth of the common air, loſe their elaſticity, and are condenſed into dew. And by this experiment it appears, that this dew is plain water, and conſequently that almoſt all the inflammable air, and about one-fifth of the common air, are turned into pure water.

In order to examine the nature of the matter condenſed on firing a mixture of dephlogiſticated and inflammable air, I took a glaſs globe, holding 8800 grain meaſures, furniſhed with a braſs cock and an apparatus for firing air by electricity. This globe was well exhauſted by an air-pump, and then filled with a mixture of inflammable and dephlogiſticated air, by ſhutting the cock, faſtening a bent glaſs tube to its mouth, and letting up the end of it into a glaſs jar inverted into water, and containing a mixture of 19500 grain meaſures of dephlogiſticated air, and 37000 of inflammable; ſo that, upon opening the cock, ſome of this mixed air ruſhed through the bent tube, and filled the globe *. The cock was then ſhut, and the included air fired by electricity, by which means almoſt all of it loſt its elaſticity. The cock was then again opened, ſo as to let in more of the ſame air, to ſupply the place of that deſtroyed by the exploſion, which was again fired, and the operation continued till almoſt the whole of the mixture was let into the globe and exploded. By this means, though the globe held not more than the ſixth part of the mixture, almoſt the whole of it was exploded therein, without any freſh exhauſtion of the globe.

As I was deſirous to try the quantity and teſt of this burnt air, without letting any water into the globe, which would have prevented my examining the nature of the condenſed matter, I took a larger globe, furniſhed alſo with a ſtop cock, exhauſted it by an air-pump, and ſcrewed it on upon the cock of the former globe; upon which, by opening both cocks, the air ruſhed out of the ſmaller globe into the larger, till it became of equal denſity in both; then, by ſhutting the cock of the larger globe, unſcrewing it again from the former, and opening it under water, I was enabled to find the quantity of the burnt air in it; and conſequently, as the proportion which the contents of the two globes bore to each other was

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* In order to prevent any water from getting into this tube, while dipped under water to let it up into the glaſs jar, a bit of wax was was ſtuck upon the end of it, which was rubbed off when raiſed above the ſurface of the water.

known, could tell the quantity of burnt air in the ſmall globe before the communication was made between them. By this means the whole quantity of the burnt air was found to be 2950 grain meaſures; its ſtandard was 1,85.

The liquor condenſed in the globe, in weight about 30 grains, was ſenſibly acid to the taſte, and by ſaturation with fixed alkali, and evaporation, yielded near two grains of nitre; ſo that it conſiſted of water united to a ſmall quantity of nitrous acid. No ſooty matter was depoſited in the globe. The dephlogiſticated air uſed in this experiment was procured from red precipitate, that is, from a ſolution of quickſilver in ſpirit of nitre diſtilled till it acquires a red colour.

As it was ſuſpected, that the acid contained in the condenſed liquor was no eſſential part of the dephlogiſticated air, but was owing to ſome acid vapour which came over in making it and had not been abſorbed by the water, the experiment was repeated in the ſame manner, with ſome more of the ſame air, which had been previouſly waſhed with water, by keeping it a day or two in a bottle with ſome water, and ſhaking it frequently; whereas that uſed in the preceding experiment had never paſſed through water, except in preparing it. The condenſed liquor was ſtill acid.

The experiment was alſo repeated with dephlogiſticated air, procured from red lead by means of oil of vitriol; the liquor condenſed was acid, but by an accident I was prevented from determining the nature of the acid.

I alſo procured ſome dephlogiſticated air from the leaves of plants, in the manner of Doctors INGENHOUSZ and PRIESTLEY, and exploded it with inflammable air as before; the condenſed liquor ſtill continued acid, and of the nitrous kind.

In all theſe experiments the proportion of inflammable air was ſuch, that the burnt air was not much phlogiſticated; and it was obſerved, that the leſs phlogiſticated it was, the more acid was the condenſed liquor. I therefore made another experiment, with ſome more of the ſame air from plants, in which the proportion of inflammable air was greater, ſo that the burnt air was almoſt completely phlogiſticated, its ſtandard being $\frac{1}{10}$. The condenſed liquor was then not at all acid, but ſeemed pure water: ſo that it appears, that with this kind of dephlogiſticated air, the condenſed liquor is not at all acid, when the two airs are mixed in ſuch a proportion that the burnt air is almoſt completely phlogiſticated, but is conſiderably ſo when it is not much phlogiſticated.

In order to ſee whether the ſame thing would obtain with air procured from red precipitate, I made two more experiments with that kind of air, the air in both being taken from the ſame bottle, and the experiment tried in the ſame manner, except that the proportions of inflammable air were different. In the firſt, in which the burnt air was almoſt completely phlogiſticated, the condenſed liquor was not at all acid. In the ſecond, in which its ſtandard was 1,86, that is, not much phlogiſticated, it was conſiderably acid; ſo that with this air, as well as with that from plants, the condenſed liquor contains, or is entirely free from, acid, according as the burnt air is leſs or more phlogiſticated; and there can be little doubt but that the ſame rule obtains with any other kind of dephlogiſticated air.

In order to ſee whether the acid, formed by the exploſion of dephlogiſticated air obtained by means of the vitriolic acid, would alſo be of the nitrous kind, I procured ſome air from turbith mineral, and exploded it with inflammable air, the proportion being ſuch that the burnt air was not much phlogiſticated. The condenſed liquor manifeſted an acidity, which appeared, by ſaturation with a ſolution of ſalt of tartar, to be of the nitrous kind; and it was found, by the addition of ſome terra ponderoſa ſalita, to contain little or no vitriolic acid.

When inflammable air was exploded with common air, in ſuch a proportion that the ſtandard of the burnt air was about $\frac{4}{10}$, the condenſed liquor was not in the leaſt acid. There is no difference, however, in this reſpect between common air, and dephlogiſticated air mixed with phlogiſticated in ſuch a proportion as to reduce it to the ſtandard of common air; for ſome dephlogiſticated air from red precipitate, being reduced to this ſtandard by the addition of perfectly phlogiſticated air, and then exploded with the ſame proportion of inflammable air as the common air was in the foregoing experiment, the condenſed liquor was not in the leaſt acid.

From the foregoing experiments it appears, that when a mixture of inflammable and dephlogiſticated air is exploded in ſuch proportion that the burnt air is not much phlogiſticated, the condenſed liquor contains a little acid, which is always of the nitrous kind, whatever ſubſtance the dephlogiſticated air is procured from; but if the proportion be ſuch that the burnt air is almoſt entirely phlogiſticated, the condenſed liquor is not at all acid, but ſeems pure water, without any addition whatever; and as, when they are mixed in that proportion, very little air remains after the exploſion, almoſt the whole being condenſed, it follows, that almoſt the whole of the inflammable and dephlogiſticated air is converted into pure water. It is not eaſy, indeed, to determine from theſe experiments what proportion the burnt air, remaining after the exploſions, bore to the dephlogiſticated air employed, as neither the ſmall nor the large globe could be perfectly exhauſted of air, and there was no ſaying with exactneſs what quantity was left in them; but in moſt of them, after allowing for this uncertainty, the true quantity of burnt air ſeemed not more than $\frac{1}{17}$th of the dephlogiſticated air employed, or $\frac{1}{50}$th of the mixture. It ſeems, however, unneceſſary to determine this point exactly, as the quantity is ſo ſmall, that there can be little doubt but that it proceeds only from the impurities mixed with the dephlogiſticated and inflammable air, and conſequently that, if thoſe airs could be obtained perfectly pure, the whole would be condenſed.

With reſpect to common air, and dephlogiſticated air reduced by the addition of phlogiſticated air to the ſtandard of common air, the caſe is different; as the liquor condenſed in exploding them with inflammable air, I believe I may ſay in any proportion, is not at all acid; perhaps, becauſe if they are mixed in ſuch a proportion as that the burnt air is not much phlogiſticated, the exploſion is too weak, and not accompanied with ſufficient heat.

All the foregoing experiments, on the exploſion of inflammable air with common and dephlogiſticated airs, except thoſe which relate to the cauſe of the acid found in the water, were made in the ſummer of the year 1781, and were mentioned by me to Dr. PRIESTLEY, who in conſequence of it made ſome experiments of the ſame kind, as he relates in a paper printed in the preceding volume of the Tranſactions. During the laſt ſummer alſo, a friend of mine gave ſome account of them to M. LAVOISIER, as well as of the concluſion drawn from them, that dephlogiſticated air is only water deprived of phlogiſton; but at that time ſo far was M. LAVOISIER from thinking any ſuch opinion warranted, that, till he was prevailed upon to repeat the experiment himſelf, he found ſome difficulty in believing that nearly the whole of the two airs could be converted into water. It is remarkable, that neither of theſe gentlemen found any acid in the water produced by the combuſtion; which might proceed from the latter having burnt the two airs in a different manner from what I did; and from the former having uſed a different kind of inflammable air, namely, that from charcoal, and perhaps having uſed a greater proportion of it.

Before I enter into the cauſe of theſe phænomena, it will be proper to take notice, that phlogiſticated air appears to be nothing elſe than the nitrous acid united to phlogiſton; for when nitre is deflagrated with charcoal, the acid is almoſt entirely converted into this kind of air. That the acid is entirely converted into air, appears from the common proceſs for making what is called clyſſus of nitre; for if the nitre and charcoal are dry, ſcarce any thing is found in the veſſels prepared for condenſing the fumes; but if they are moiſt a little liquor is collected, which is nothing but the water contained in the materials, impregnated with a little volatile alkali, proceeding in all probability from the imperfectly burnt charcoal, and a little fixed alkali, conſiſting of ſome of the alkalized nitre carried over by the heat and watery vapours. As far as I can perceive too, at preſent, the air into which much the greateſt part of the acid is converted, differs in no reſpect from common air phlogiſticated. A ſmall part of the acid, however, is turned into nitrous air, and the whole is mixed with a good deal of fixed, and perhaps a little inflammable air, both proceeding from the charcoal.

It is well known, that the nitrous acid is alſo converted by phlogiſtication into nitrous air, in which reſpect there ſeems a conſiderable analogy between that and the vitriolic acid; for the vitriolic acid, when united to a ſmaller proportion of phlogiſton, forms the volatile ſulphureous acid and vitriolic acid air, both of which, by expoſure to the atmoſphere, loſe their phlogiſton, though not very faſt, and are turned back into vitriolic acid; but, when united to a greater proportion of phlogiſton, it forms ſulphur, which ſhews no ſigns of acidity, unleſs a ſmall degree of affinity to alkalies can be called ſo, and in which the phlogiſton is more ſtrongly adherent, ſo that it does not fly off when expoſed to the air, unleſs aſſiſted by a heat ſufficient to ſet it on fire. In like manner the nitrous acid, united to a certain quantity of phlogiſton, forms nitrous fumes and nitrous air, which readily quit their phlogiſton to common air; but when united to a different, in all probability a larger quantity, it forms phlogiſticated air, which ſhews no ſigns of acidity, and is ſtill leſs diſpoſed to part with its phlogiſton than ſulphur.

This being premiſed, there ſeem two ways by which the phænomena of the acid found in the condenſed liquor may be explained; firſt, by ſuppoſing that dephlogiſticated air contains a little nitrous acid which enters into it as one of its component parts, and that this acid, when the inflammable air is in a ſufficient proportion, unites to the phlogiſton, and is turned into phlogiſticated air, but does not when the inflammable air is in too ſmall a proportion; and, ſecondly, by ſuppoſing that there is no nitrous acid mixed with, or entering into the compoſition of, dephlogiſticated air, but that, when this air is in a ſufficient proportion, part of the phlogiſticated air with which it is debaſed is, by the ſtrong affinity of phlogiſton to dephlogiſticated air, deprived of its phlogiſton and turned into nitrous acid; whereas, when the dephlogiſticated air is not more than ſufficient to conſume the inflammable air, none then remains to deprive the phlogiſticated air of its phlogiſton, and turn it into acid.

If the latter explanation be true, I think, we muſt allow that dephlogiſticated air is in reality nothing but dephlogiſticated water, or water deprived of its phlogiſton; or, in other words, that water conſiſts of dephlogiſticated air united to phlogiſton; and that inflammable air is either pure phlogiſton, as Dr. PRIESTLEY and Mr. KIRWAN ſuppoſe, or elſe water united to phlogiſton *; ſince, according to this ſuppoſition, theſe two ſubſtances united together form pure water. On the other hand, if the firſt explanation be true, we muſt ſuppoſe that dephlogiſticated air conſiſts of water united to a little nitrous acid and deprived of its phlogiſton; but ſtill the nitrous acid in it muſt make only a very ſmall part of the whole,

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* Either of theſe ſuppoſitions will agree equally well with the following experiments; but the latter ſeems to me much the moſt likely. What principally makes me think ſo is, that common or dephlogiſticated air do not abſorb phlogiſton from inflammable air, unleſs aſſiſted by a red heat, whereas they abſorb the phlogiſton of nitrous air, liver of ſulphur, and many other ſubſtances, without that aſſiſtance; and it ſeems inexplicable, that they ſhould refuſe to unite to pure phlogiſton, when they are able to extract it from ſubſtances to which it has an affinity; that is, that they ſhould overcome the affinity of phlogiſton to other ſubſtances, and extract it from them, when they will not even unite to it when preſented to them. On the other hand, I know no experiment which ſhews inflammable air to be pure phlogiſton rather than an union of it with water, unleſs it be Dr. PRIESTLEY'S experiment of expelling inflammable air from iron by heat alone. I am not ſufficiently acquainted with the circumſtances of that experiment to argue with certainty about it; but I think it much more likely, that the inflammable air was formed by the union of the phlogiſton of the iron filings with the water diſperſed among them, or contained in the retort or other veſſel in which it was heated; and in all probability this was the cauſe of the ſeparation of the phlogiſton, as iron ſeems not diſpoſed to part with its phlogiſton by heat alone, without being aſſiſted by the air or ſome other ſubſtance.

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as it is found, that the phlogiſticated air, which it is converted into, is very ſmall in compariſon of the dephlogiſticated air.

I think the ſecond of theſe explanations ſeems much the moſt likely; as it was found, that the acid in the condenſed liquor was of the nitrous kind, not only when the dephlogiſticated air was prepared from red precipitate, but alſo when it was procured from plants or from turbith mineral: and it ſeeems not likely, that air procured from plants, and ſtill leſs likely that air procured from a ſolution of mercury in oil of vitriol, ſhould contain any nitrous acid.

Another ſtrong argument in favour of this opinion is, that dephlogiſticated air yields no nitrous acid when phlogiſticated by liver of ſulphur; for if this air contains nitrous acid, and yields it when phlogiſticated by exploſion with inflammable air, it is very extraordinary that it ſhould not do ſo when phlogiſticated by other means.

But what forms a ſtronger and, I think, almoſt deciſive argument in favour of this explanation is, that when the dephlogiſticated air is very pure, the condenſed liquor is made much more ſtrongly acid by mixing the air to be exploded with a little phlogiſticated air, as appears by the following experiments.

A mixture of 18500 grain meaſures of inflammable air with 9750 of dephlogiſticated air procured from red precipitate were exploded in the uſual manner; after which, a mixture of the ſame quantities of the ſame dephlogiſticated and inflammable air, with the addition of 2500 of air phlogiſticated by iron filings and ſulphur, was treated in the ſame manner. The condenſed liquor, in both experiments, was acid, but that in the latter evidently more ſo, as appeared alſo by ſaturating each of them ſeparately with marble powder, and precipitating the earth by fixed alkali, the precipitate of the ſecond experiment weighing one-fifth of a grain, and that of the firſt being ſeveral times leſs. The ſtandard of the burnt air in the firſt experiment was 1,86, and in the ſecond only 0,9.

It muſt be obſerved, that all circumſtances were the ſame in theſe two experiments, except that in the latter the air to be exploded was mixed with ſome phlogiſticated air, and that in conſequence the burnt air was more phlogiſticated than in the former; and from what has been before ſaid, it appears, that this latter circumſtance ought rather to have made the condenſed liquor leſs acid; and yet it was found to be much more ſo, which ſhews ſtrongly that it was the phlogiſticated air which furniſhed the acid.

As a further confirmation of this point, theſe two comparative experiments were repeated with a little variation, namely, in the firſt experiment there was firſt let into the globe 1500 of dephlogiſticated air, and then the mixture, conſiſting of 12200 of dephlogiſticated air and 25900 of inflammable, was let in at different times as uſual. In the ſecond experiment, beſides the 1500 of dephlogiſticated air firſt let in, there was alſo admitted 2500 of phlogiſticated air, after which the mixture, conſiſting of the ſame quantities of dephlogiſticated and inflammable air as before, was let in as uſual. The condenſed liquor of the ſecond experiment was about three times as acid as that of the firſt, as it required 119 grains of a diluted ſolution of ſalt of tartar to ſaturate it, and the other only 37. The ſtandard of the burnt air was 0,78 in the ſecond experiment, and 1,96 in the firſt.

The intention of previouſly letting in ſome dephlogiſticated air in the two laſt experiments was, that the condenſed liquor was expected to become more acid thereby, as proved actually to be the caſe.

In the firſt of theſe two experiments, in order that the air to be exploded ſhould be as free as poſſible from common air, the globe was firſt filled with a mixture of dephlogiſticated and inflammable air, it was then exhauſted, and the air to be exploded let in; by which means, though the globe was not perfectly exhauſted, very little common air could be left in it. In the firſt ſet of experiments this circumſtance was not attended to, and the purity of the dephlogiſticated air was forgot to be examined in both ſets.

From what has been ſaid there ſeems the utmoſt reaſon to think, that dephlogiſticated air is only water deprived of its phlogiſton, and that inflammable air, as was before ſaid, is either phlogiſticated water, or elſe pure phlogiſton; but in all probability the former.

As Mr. WATT, in a paper lately read before this Society, ſuppoſes water to conſiſt of dephlogiſticated air and phlogiſton deprived of part of their latent heat, whereas I take no notice of the latter circumſtance, it may be proper to mention in a few words the reaſon of this apparent difference betwen us. If there be any ſuch thing as elementary heat, it muſt be allowed that what Mr. WATT ſays is true; but by the ſame rule we ought to ſay, that the diluted mineral acids conſiſt of the concentrated acids united to water and deprived of part of their latent heat; that ſolutions of ſal ammoniac, and moſt other other neutral ſalts, conſiſt of the ſalt united to water and elementary heat; and a ſimilar language ought to be uſed in ſpeaking of almoſt all chemical combinations, as there are very few which are not attended with ſome increaſe or diminution of heat. Now I have choſen to avoid this form of ſpeaking, both becauſe I think it more likely that there is no ſuch thing as elementary heat, and becauſe ſaying ſo in this inſtance, without uſing ſimilar expreſſions in ſpeaking of other chemical unions, would be improper, and would lead to falſe ideas; and it may even admit of doubt, whether the doing it in general would not cauſe more trouble and perplexity than it is worth.

There is the utmoſt reaſon to think, that dephlogiſticated and phlogiſticated air, as M. LAVOISIER and SCHEELE ſuppoſe, are quite diſtinct ſubſtances, and not differing only in their degree of phlogiſtication; and that common air is a mixture of the two; for if the dephlogiſticated air is pretty pure, almoſt the whole of it loſes its elaſticity by phlogiſtication, and, as appears by the foregoing experiments, is turned into water, inſtead of being converted into phlogiſticated air. In moſt of the foregoing experiments, at leaſt $\frac{16}{17}$ths of the whole was turned into water; and by treating ſome dephlogiſticated air with liver of ſulphur, I have reduced it to leſs than $\frac{1}{30}$th of its original bulk, and other perſons, I believe, have reduced it to a ſtill leſs bulk; ſo that there ſeems the utmoſt reaſon to ſuppoſe, that the ſmall reſiduum which remains after its phlogiſtication proceeds only from the impurities mixed with it.

It was juſt ſaid, that ſome dephlogiſticated air was reduced by liver of ſulphur to $\frac{1}{30}$th of its original bulk; the ſtandard of this air was 4,8, and conſequently the ſtandard of perfectly pure dephlogiſticated air ſhould be very nearly 5, which is a confirmation of the foregoing opinion; for if the ſtandard of pure dephlogiſticated air is 5, common air muſt, according to this opinion, contain one-fifth of it, and therefore ought to loſe one-fifth of its bulk by phlogiſtication, which is what it is actually found to loſe.

From what has been ſaid, it follows, that inſtead of ſaying air is phlogiſticated or dephlogiſticated by any means, it would be more ſtrictly juſt to ſay, it is deprived of, or receives, an addition of dephlogiſticated air; but as the other expreſſion is convenient, and can ſcarcely be conſidered as improper, I ſhall ſtill frequently make uſe of it in the remainder of this paper.

There ſeemed great reaſon to think, from Dr. PRIESTLEY's experiments, that both the nitrous and vitriolic acids were convertible into dephlogiſticated air, as that air is procured in the greateſt quantity from ſubſtances containing thoſe acids, eſpecially the former. The foregoing experiments, however, ſeem to ſhew that no part of the acid is converted into dephlogiſticated air, and that their uſe in preparing it is owing only to the great power which they poſſeſs of depriving bodies of their phlogiſton. A ſtrong confirmation of this is, that red precipitate, which is one of the ſubſtances yielding dephlogiſticated air in the greateſt quantity, and which is prepared by means of the nitrous acid, contains in reality no acid. This I found by grinding 400 grains of it with ſpirits of ſal ammoniac, and keeping them together for ſome days in a bottle, taking care to ſhake them frequently. The red colour of the precipitate was rendered pale, but not entirely deſtroyed; being then waſhed with water and filtered, the clear liquor yielded on evaporation not the leaſt ammoniacal ſalt.

It is natural to think, that if any nitrous acid had been contained in the red precipitate, it would have united to the volatile alkali and have formed ammoniacal nitre, and would have been perceived on evaporation; but in order to determine more certainly whether this would be the caſe, I dried ſome of the ſame ſolution of quickſilver from which the red precipitate was prepared with a leſs heat, ſo that it acquired only an orange colour, and treated the ſame quantity of it with volatile alkali in the ſame manner as before. It immediately cauſed an effervefcence, changed the colour to grey, and yielded 52 grains of ammoniacal nitre. There is the utmoſt reaſon to think, therefore, that red precipitate contains no nitrous acid; and conſequently that, in procuring dephlogiſticated air from it, no acid is converted into air; and it is reaſonable to conclude, therefore, that no ſuch change is produced in procuring it from any other ſubſtance.

It remains to conſider in what manner theſe acids act in producing dephlogiſticated air. The way in which the nitrous acid acts, in the production of it from red precipitate, ſeems to be as follows. On diſtilling the mixture of quickſilver and ſpirit of nitre, the acid comes over, loaded with phlogiſton, in the form of nitrous vapour, and continues to do ſo till the remaining matter acquires its full red colour, by which time all the nitrous acid is driven over, but ſome of the watery part ſtill remains behind, and adheres ſtrongly to the quickſilver; ſo that the red precipitate may be conſidered, either as quickſilver deprived of part of its phlogiſton, and united to a certain portion of water, or as quickſilver united to dephlogiſticated air*; after which, on further increaſing the heat, the water in it riſes deprived of its phlogiſton, that is, in the form of dephlogiſticated air, and at the ſame time the quickſilver diſtils over in its metallic form. It is juſtly remarked by Dr. PRIESTLEY, that the ſolution of quickſilver does not begin to yield dephlogiſticated air till it acquires its red colour.

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* Unleſs we were much better acquainted than we are with the manner in which different ſubſtances are united together in compound bodies, it would be ridiculous to ſay, that it is the quickſilver in the red precipitate which is deprived of its phlogiſton, and not the water, or that it is the water and not the quickſilver; all that we can ſay is, that red precipitate conſiſts of quickſilver and water, one or both of which are deprived of part of their phlogiſton. In like manner, during the preparation of the red precipitate, it is certain that the acid abſorbs phlogiſton, either from the quickſilver or the water; but we are by no means authoriſed to ſay from which.

Mercurius calcinatus appears to be only quickſilver which has abſorbed dephlogiſticated air from the atmoſphere during its preparation; accordingly, by giving it a ſufficient heat, the dephlogiſticated air is driven off, and the quickſilver acquires its original form. It ſeems therefore that mercurius calcinatus and red precipitate, though prepared in a different manner, are very nearly the ſame thing.

From what has been ſaid it follows, that red precipitate and mercurius calcinatus contain as much phlogiſton as the quickſilver they are prepared from; but yet, as uniting dephlogiſticated air to a metal comes to the ſame thing as depriving it of part of its phlogiſton and adding water to it, the quickſilver may ſtill be conſidered as deprived of its phlogiſton; but the imperfect metals ſeem not only to abſorb dephlogiſticated air during their calcination, but alſo to be really deprived of part of their phlogiſton, as they do not acquire their metallic form by driving off the dephlogiſticated air.

In procuring dephlogiſticated air from nitre, the acid acts in a different manner, as, upon heating the nitre red-hot, the dephlogiſticated air riſes mixed with a little nitrous acid, and at the ſame time the acid remaining in the nitre becomes very much phlogiſticated; which ſhews that the acid abſorbs phlogiſton from the water in the nitre, and becomes phlogiſticated, while the water is thereby turned into dephlogiſticated air. On diſtilling 3155 grains of nitre in an unglazed earthen retort, it yielded 256000 grain meaſures of dephlogiſticated air*, the ſtandard of different parts of which varied from 3 to 3,65, but at a medium was 3,35. The matter remaining in the retort diſſolved readily in water, and taſted alcaline and cauſtic. On adding diluted ſpirit of nitre to the ſolution, ſtrong red fumes were produced; a ſign that the acid in it was very much phlogiſticated, as no fumes whatever would have been produced on adding the ſame acid to a ſolution of common nitre; that part of the ſolution alſo which was ſuperſaturated with acid became blue; a colour which the diluted nitrous acid is known to aſſume when much phlogiſticated. The ſolution, when ſaturated with this acid, loſt its alcaline and cauſtic taſte, but yet taſted very different from true nitre, ſeeming as if it had been mixed with ſea-ſalt, and alſo required much leſs water to diſſolve it; but on expoſing it for ſome days to the air, and adding freſh acid as faſt as by the flying off of the fumes the alcali predominated, it became true nitre, unmixed, as far as I could perceive, with any other ſalt *.

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* This is, about eighty-one grain meaſures from one grain of nitre; and the weight

It has been remarked, that the dephlogiſticated air procured from nitre is leſs pure, than that from red precipitate and many other ſubſtances, which may perhaps proceed from unglazed earthen retorts having been commonly uſed for this purpoſe, and which, conformably to Dr. PRIESTLEY's diſcovery, may poſſibly abſorb ſome common air from without, and emit it along with the dephlogiſticated air; but if it ſhould be found that the dephlogiſticated air procured from nitre in glaſs or glazed earthen veſſels is alſo impure, it would ſeem to ſhew that part

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weight of the dephlogiſticated air, ſuppoſing it 800 times lighter than water, is one tenth of that of the nitre. In all probability it would have yielded a much greater quantity of air, if a greater heat had been applied.

* This phlogiſtication of the acid in nitre by heat has been obſerved by Mr. SCHEELE; ſee his experiments on air and fire, p. 45. Engliſh tranſlation.

of the acid in the nitre is turned into phlogiſticated air, by abſorbing phlogiſton from the watery part.

From what has been ſaid it appears, that there is a conſiderable difference in the manner in which the acid acts in the production of dephlogiſticated air from red precipitate and from nitre; in the former caſe the acid comes over firſt, leaving the remaining ſubſtance deprived of part of its phlogiſton; in the latter the dephlogiſticated air comes firſt, leaving the acid loaded with the phlogiſton of the water from which it was formed.

On diſtilling a mixture of quickſilver and oil of vitriol to dryneſs, part of the acid comes over, loaded with phlogiſton, in the form of volatile ſulphureous acid and vitriolic acid air; ſo that the remaining white maſs may be conſidered as conſiſting of quickſilver deprived of its phlogiſton, and united to a certain proportion of acid and water, or of plain quickſilver united to a certain proportion of acid and dephlogiſticated air. Accordingly on urging this white maſs with a more violent heat, the dephlogiſticated air comes over, and at the ſame time part of the quickſilver riſes in its metallic form, and alſo part of the white maſs, united in all probability to a greater proportion of acid than before, ſublimes; ſo that the rationale of the production of dephlogiſticated air from turbith mineral, and from red precipitate, are nearly ſimilar.

True turbith mineral conſiſts of the abovementioned white maſs, well waſhed with water, by which means it acquires a yellow colour, and contains much leſs acid than the unwaſhed maſs. Accordingly it ſeems likely, that on expoſing this to heat, leſs of it ſhould ſublime without being decompounded, and conſequently that more dephlogiſticated air ſhould be procured from it than from the unwaſhed maſs.

This is an inſtance, that the ſuperabundant vitriolic acid may, in ſome caſes, be better extracted from the baſe it is united to by water than by heat. Vitriolated tartar is another inſtance; for, if vitriolated tartar be mixed with oil of vitriol and expoſed even to a pretty ſtrong red heat, the maſs will be very acid; but, if this maſs is diſſolved in water, and evaporated, the cryſtals will be not ſenſibly ſo.

In all probability, the vitriolic acid acts in the ſame manner in the production of dephlogiſticated air from alum, as the nitrous does in its production from nitre; that is, the watery part comes over firſt in the form of dephlogiſticated air, leaving the acid charged with its phlogiſton. Whether this is alſo the caſe with regard to green and blue vitriol, or whether in them the acid does not rather act in the ſame manner as in turbith mineral, I cannot pretend to ſay, but I think the latter more likely.

There is another way by which dephlogiſticated air has been found to be produced in great quantities, namely, the growth of vegetables expoſed to the ſun or day-light; the rationale of which, in all probability, is, that plants, when aſſiſted by the light, deprive part of the water ſucked up by their roots of its phlogiſton, and turn it into dephlogiſticated air, while the phlogiſton unites to, and forms part of, the ſubſtance of the plant.

There are many circumſtances which ſhew, that light has a remarkable power in enabling one body to abſorb phlogiſton from another. Mr. SENEBIER has obſerved, that the green tincture procured from the leaves of vegetables by ſpirit of wine, quickly loſes its colour when expoſed to the ſun in a bottle not more than one third part full, but does not do ſo in the dark, or if the bottle is quite full of the tincture, or if the air in it is phlogiſticated; whence it is natural to conclude, that the light enables the dephlogiſticated part of the air to abſorb phlogiſton from the tincture; and this appears to be really the caſe, as I find that the air in the bottle is conſiderably phlogiſticated thereby. Dephlogiſticated ſpirit of nitre alſo acquires a yellow colour, and becomes phlogiſticated, by expoſure to the ſun's rays *; and I find on trial that the air in the bottle in which it is contained becomes dephlogiſticated, or, in other words, receives an increaſe of dephlogiſticated air, which ſhews that the change in the acid is not owing to the ſun's rays communicating phlogiſton to it, but to their enabling it to abſorb phlogiſton from the water contained in it, and thereby to produce dephlogiſticated air. Mr. Scheele alſo found, that the dark colour acquired by luna cornea on expoſure to the light, is owing to part of the ſilver being revived; and that gold, diſſolved in aqua regia and deprived by diſtillation of the nitrous and ſuperfluous marine acid, is revived by the ſame means; and there is the utmoſt reaſon to think, that, in both caſes, the revival of the metal is owing to its abſorbing phlogiſton from the water.

Vegetables ſeem to conſiſt almoſt intirely of fixed and phlogiſticated air, united to a large proportion of phlogiſton and ſome water, ſince by burning in the open air, in which their phlogiſton unites to the dephlogiſticated part of the atmoſphere and forms water, they ſeem to be reduced almoſt intirely to water and thoſe two kinds of air. Now plants growing in water without earth, can receive nouriſhment only from the water and air, and muſt therefore in all probability abſorb their phlogiſton from the water. It is known alſo that plants growing in the dark do not thrive well, and grow in a very different manner from what they do when expoſed to the light.

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* If ſpirit of nitre is diſtilled with a very gentle heat, the part which comes over is high coloured and fuming, and that which remains behind is quite colourleſs, and fumes much leſs than other nitrous acid of the ſame ſtrength, and the fumes are colourleſs. This is called dephlogiſticated ſpirit of nitre, as it appears to be really deprived of phlogiſton by the proceſs. The manner of preparing it, as well as its property of regaining its yellow colour by expoſure to the light, is mentioned by Mr. Scheele in the Stockholm Memoirs, 1774.

From what has been ſaid, it ſeems likely that the uſe of light, in promoting the growth of plants and the production of dephlogiſticated air from them, is, that it enables them to abſorb phlogiſton from the water. To this it may perhaps be objected, that though plants do not thrive well in the dark, yet they do grow, and ſhould therefore, according to this hypotheſis, abſorb water from the atmoſphere, and yield dephlogiſticated air, which they have not been found to do. But we have no proof that they grew at all in any of thoſe caſes in which they were found not to yield dephlogiſticated air; for though they will grow in the dark, yet their vegetative powers may perhaps at firſt be intirely checked by it, eſpecially conſidering the unnatural ſituation in which they muſt be placed in ſuch experiments. Perhaps too plants growing in the dark may be able to abſorb phlogiſton from water not much impregnated with dephlogiſticated air, but not from water ſtrongly impregnated with it; and conſequently, when kept under water in the dark, may perhaps at firſt yield ſome dephlogiſticated air, which, inſtead of riſing to the ſurface, may be abſorbed by the water, and, before the water is ſo much impregnated as to ſuffer any to eſcape, the plant may ceaſe to vegetate, unleſs the water is changed. Unleſs therefore it could be ſhewn that plants growing in the dark, in water alone, will increaſe in ſize, without yielding dephlogiſticated air, and without the water becoming more impregnated with it than before, no objection can be drawn from thence.

Mr. SENEBIER finds, that plants yield much more dephlogiſticated air in diſtilled water impregnated with fixed air, than in plain diſtilled water, which is perfectly conformable to the abovementioned hypotheſis; for as fixed air is a principal conſtituent part of vegetable ſubſtances, it is reaſonable to ſuppoſe that the work of vegetation will go on better in water containing this ſubſtance, than in other water.

There are ſeveral memoirs of Mr. LAVOISIER publiſhed by the Academy of Sciences, in which he intirely diſcards phlogiſton, and explains thoſe phænomena which have been uſually attributed to the loſs or attraction of that ſubſtance, by the abſorption or expulſion of dephlogiſticated air; and as not only the foregoing experiments, but moſt other phænomena of nature, ſeem explicable as well, or nearly as well, upon this as upon the commonly believed principle of phlogiſton, it may be proper briefly to mention in what manner I would explain them on this principle, and why I have adhered to the other. In doing this, I ſhall not conform ſtrictly to his theory, but ſhall make ſuch additions and alterations as ſeem to ſuit it beſt to the phænomena; the more ſo, as the foregoing experiments may, perhaps, induce the author himſelf to think ſome ſuch additions proper.

According to this hypotheſis, we muſt ſuppoſe, that water conſiſts of inflammable air united to dephlogiſticated air; that nitrous air, vitriolic acid air, and the phoſphoric acid, are alſo combinations of phlogiſticated air, ſulphur, and phoſphorus, with dephlogiſticated air; and that the two former, by a further addition of the ſame ſubſtance, are reduced to the common nitrous and vitriolic acids; that the metallic calces conſiſt of the metals themſelves united to the ſame ſubſtance, commonly, however, with a mixture of fixed air; that on expoſing the calces of the perfect metals to a ſufficient heat, all the dephlogiſticated air is driven off, and the calces are reſtored to their metallic form; but as the calces of the imperfect metals are vitrified by heat, inſtead of recovering the metallic form, it ſhould ſeem as if all the dephlogiſticated air could not be driven off from them by heat alone. In like manner, according to this hypotheſis, the rationale of the production of dephlogiſticated air from red precipitate is, that during the ſolution of the quickſilver in the acid and the ſubſequent calcination, the acid is decompounded, and quits part of its dephlogiſticated air to the quickſilver, whereby it comes over in the form of nitrous air, and leaves the quickſilver behind united to dephlogiſticated air, which, by a further increaſe of heat, is driven off, while the quickſilver re-aſſumes its metallic form. In procuring dephlogiſticated air from nitre, the acid is alſo decompounded; but with this difference, that it ſuffers ſome of its dephlogiſticated air to eſcape, while it remains united to the alkali itſelf, in the form of phlogiſticated nitrous acid. As to the production of dephlogiſticated air from plants, it may be ſaid, that vegetable ſubſtances conſiſt chiefly of various combinations of three different baſes, one of which, when united to dephlogiſticated air, forms water, another fixed air, and the third phlogiſticated air; and that by means of vegetation each of theſe ſubſtances are decompoſed, and yield their dephlogiſticated air; and that in burning they again acquire dephlogiſticated air, and are reſtored to their priſtine form.

It ſeems, therefore, from what has been ſaid, as if the phænomena of nature might be explained very well on this principle,

ciple, without the help of phlogiſton; and indeed, as adding dephlogiſticated air to a body comes to the ſame thing as depriving it of its phlogiſton and adding water to it, and as there are, perhaps, no bodies entirely deſtitute of water, and as I know no way by which phlogiſton can be transferred from one body to another, without leaving it uncertain whether water is not at the ſame time transferred, it will be very difficult to determine by experiment which of theſe opinions is the trueſt; but as the commonly received principle of phlogiſton explains all phænomena, at leaſt as well as Mr. LAVOISIER's, I have adhered to that. There is one circumſtance alſo, which though it may appear to many not to have much force, I own has ſome weight with me; it is, that as plants ſeem to draw their nouriſhment almoſt intirely from water and fixed and phlogiſticated air, and are reſtored back to thoſe ſubſtances by burning, it ſeems reaſonable to conclude, that notwithſtanding their infinite variety they conſiſt almoſt intirely of various combinations of water and fixed and phlogiſticated air, united according to one of theſe opinions to phlogiſton, and deprived according to the other of dephlogiſticated air; ſo that, according to the latter opinion, the ſubſtance of a plant is leſs compounded than a mixture of thoſe bodies into which it is reſolved by burning; and it is more reaſonable to look for great variety in the more compound than in the more ſimple ſubſtance.

Another thing which Mr. LAVOISIER endeavours to prove is, that dephlogiſticated air is the acidifying principle. From what has been explained it appears, that this is no more than ſaying, that acids loſe their acidity by uniting to phlogiſton, which with regard to the nitrous, vitriolic, phoſphoric, and arſenical acids is certainly true. The ſame thing, I believe, may be ſaid of the acid of ſugar; and Mr. LAVOISIER's experiment is a ſtrong confirmation of **BERGMAN**'s opinion, that none of the ſpirit of nitre enters into the compoſition of the acid, but that it only ſerves to deprive the ſugar of part of its phlogiſton. But as to the marine acid and acid of tartar, it does not appear that they are capable of loſing their acidity by any union with phlogiſton. It is to be remarked alſo, that the acids of ſugar and tartar, and in all probability almoſt all the vegetable and animal acids, are by burning reduced to fixed and phlogiſticated air, and water, and therefore contain more phlogiſton, or leſs dephlogiſticated air, than thoſe three ſubſtances.

Anſwer to Mr. Kirwan's *Remarks upon the Experiments on Air. By* Henry Cavendiſh, *Eſq. F. R. S. and S. A.*

Read at the Royal Society, March 4, 1784.

IN a paper lately read before this Society, containing many experiments on air, I gave my reaſons for ſuppoſing that the diminution which reſpirable air ſuffers by phlogiſtication, is not owing either to the generation or ſeparation of fixed air from it; but without any arguments of a perſonal nature, or which related to any one perſon who eſpouſes the contrary doctrine more than to another. This being contrary to the opinion maintained by Mr. KIRWAN, he has written a paper in anſwer to it, which was read on the fifth of February. As I do not like troubling the Society with controverſy, I ſhall take no notice of the arguments uſed by him, but ſhall leave them for the reader to form his own judgement of; much leſs will I endeavour to point out any inconſiſtencies or falſe reaſonings, ſhould any ſuch have crept into it; but as there are two or three experiments mentioned there, which may perhaps be conſidered as diſagreeing with my opinion, I beg leave to ſay a few words concerning them.

Mr. DE LASSONE found that filings of zinc, digeſted in a cauſtic fixed alkali, were partially diſſolved with a ſmall effervеſcence, and that the alkali was rendered in ſome meaſure mild. This mildneſs of the alkali Mr. KIRWAN accounts for by ſuppoſing, that the inflammable air, which is ſeparated during the ſolution, and cauſes the efferveſcence, unites to the atmoſpheric air contiguous to it, and thereby generates fixed air, which is abſorbed by the alkali. But, in reality, the only circumſtance from which Mr. DE LASSONE judged the alkali to become mild, was its making ſome efferveſcence when ſaturated with acids; and this efferveſcence is more likely to have proceeded from the expulſion of inflammable air than of fixed air, as it ſeems likely, that the zinc might be more completely deprived of its phlogiſton by the acid than by the alkali.

In the abovementioned paper I ſay, Dr. PRIESTLEY obſerved, that quickſilver fouled by the addition of lead or tin, depoſits a powder by agitation and expoſure to the air, which conſiſts in great meaſure of the calx of the imperfect metal. He found too ſome powder of this kind to contain fixed air; but it muſt be obſerved, that the powder uſed in this experiment was not prepared on purpoſe, but was procured from quickſilver fouled by having been uſed in various experiments, and may therefore have contained other impurities beſides the metallic calces. On this Mr. KIRWAN remarks, that Dr. PRIESTLEY did not at firſt prepare this powder on purpoſe, but he afterwards did ſo prepare it (4 PR. p. 148. and 149.), and obtained a powder exactly of the ſame ſort. It was natural to ſuppoſe from this remark, that Dr. PRIESTLEY muſt have obtained fixed air from the powder prepared on purpoſe, and that I had overlooked the paſſage; but, on turning to the pages referred to, I was ſurpriſed to find that it was otherwiſe, and that Dr. PRIESTLEY not ſo much as hints that he procured fixed air from the powder thus prepared.

With regard to the calcination of metals it may be proper to remark, that this operation is uſually performed over the fire, by methods in which they are expoſed to the fumes of the burning fuel, and which are ſo replete with fixed air, that it is not extraordinary, that the metallic calx ſhould, in a ſhort time, abſorb a conſiderable quantity of it; and in particular red lead, which is the calx on which moſt experiments have been made, is always ſo prepared. There is another kind of calcination, however, called ruſting, which is performed in the open air; but this is ſo ſlow an operation, that the ruſt may eaſily imbibe a ſufficient quantity of fixed air, notwithſtanding the ſmall quantity of it uſually contained in the atmoſphere.

Mr. KIRWAN allows that lime-water is not rendered cloudy by the mixture of nitrous and common air; but contends that this does not prove that fixed air is not generated by the union, as he thinks it may be abſorbed by the nitrous ſelenite produced by the union of the nitrous acid with the lime. This induced me to try how ſmall a quantity of fixed air would be perceived in this experiment. I accordingly repeated it in the ſame manner as deſcribed in my paper, except that I purpoſely added a little fixed air to the common air, and found that when this addition was $\frac{1}{75}$th of the bulk, or $\frac{1}{50}$th of the weight of the common air, the effect on the lime-water was ſuch as could not poſſibly have been overlooked in my experiments. But as thoſe who ſuppoſe fixed air to be generated by the mixture of nitrous and common air, may object to this manner of trying the experiment, and ſay, that the quantity of fixed air abſorbed by the lime-water was really more than $\frac{1}{75}$th of the bulk of the common air, being equal to that quantity over and above the air generated by the mixture, I made another experiment in a different manner; namely, I filled a bottle with lime-water, previouſly mixed with as much nitrous acid as is contained in an equal bulk of nitrous air, and having inverted it into a veſſel of the ſame, let up into it, in the ſame manner as in the above-mentioned experiments, a mixture of common air with $\frac{1}{75}$th of its bulk of fixed air, until it was half full. The event was the ſame as before; namely, the cloudineſs produced in the lime-water was ſuch that I could not poſſibly have overlooked. It muſt be obſerved, that in this experiment no fixed air could be generated, and a ſtill greater proportion of the lime-water was turned into nitrous ſelenite than in the above-mentioned experiments; ſo that we may ſafely conclude, that if any fixed air is generated by the mixture of common and nitrous air, it muſt be leſs than $\frac{1}{75}$th of the bulk of the common air.

As for the nitrous ſelenite, it ſeems not to make the effect of the fixed air at all leſs ſenſible, as I found by filling two bottles with common air mixed with $\frac{1}{100}$dth of its bulk of fixed air, and pouring into each of them equal quantities of diluted lime-water; one of theſe portions of lime-water being previouſly diluted with an equal quantity of diſtilled water, and the other with the ſame quantity of a diluted ſolution of nitrous ſelenite, containing about $\frac{1}{400}$dth of its weight of calcareous earth; when I could not perceive that the latter portion of lime-water was rendered at all leſs cloudy than the former. Though the nitrous ſelenite, however, does not make the effect of the fixed air leſs ſenſible, yet the dilution of the lime-water, in conſequence of ſome of the lime being abſorbed by the acid, does; but, I believe, not in any remarkable degree.

There is an experiment mentioned by Mr. KIRWAN which, though it cannot be conſidered as an argument in favour of the generation of fixed air, as he only ſuppoſes, without any proof, that fixed air is produced in it, does yet deſerve to be taken notice of as a curious experiment. It is, that, if nitrous and common air be mixed over dry quickſilver, the common air is not at all diminiſhed, that is, the bulk of the mixture will be not leſs than that of the common air employed, until water is admitted, and the mixture agitated for a few minutes. The reaſon of this in all probability is, that part of the phlogiſticated nitrous acid, into which the nitrous air is converted, remains in the ſtate of vapour until condenſed by the addition of water. A proof that this is the real caſe is, that, in this manner of performing the experiment, the red fumes produced on mixing the airs remain viſible for ſome hours, but immediately diſappear on the addition of water and agitation.

The moſt material experiment alledged by Mr. KIRWAN is one of Dr. PRIESTLEY's, in which he obtained fixed air from a mixture of red precipitate and iron filings. This at firſt ſeems really a ſtrong argument in favour of the generation of fixed air; for though plumbago, which is known to conſiſt chiefly of that ſubſtance, has lately been found to be contained in iron, yet one would not have expected it to be decompounded by the red precipitate, eſpecially when the quantity of pure iron in the filings was much more than ſufficient to ſupply the precipitate with phlogiſton. The following experiment, however, ſhews that it was really decompounded; and that the fixed air obtained was not generated, but only ſeparated by means of this decompoſition.

500 grains of red precipitate mixed with 1000 of iron filings yielded, by the aſſiſtance of heat, 7800 grain meaſures of fixed

air, beſides 2400 of a mixture of dephlogiſticated and inflammable air, but chiefly the latter. The ſame quantity of iron filings, taken from the ſame parcel, was then diſſolved in diluted oil of vitriol, ſo as to leave only the plumbago and other impurities. Theſe mixed with 500 grains of the ſame red precipitate, and treated as before, yielded 9200 grain meaſures of fixed air, and 4200 of dephlogiſticated air, of an indifferent quality, but without any ſenſible mixture of inflammable air. It appears, therefore, that leſs fixed air was produced when the red precipitate was mixed with the iron filings in ſubſtance, than when mixed only with the plumbago and other impurities; which ſhews, that its production was not owing to the iron itſelf, which ſeems to contain no fixed air, but to the plumbago, which contains a great deal. The reaſon, in all probability, why leſs fixed air was produced in the firſt caſe than the latter is, that in the former more of the plumbago eſcaped being decompounded by the red precipitate than in the other. It muſt be obſerved, however, that the filings uſed in this experiment were mixed with about $\frac{1}{13}$th of their weight of braſs, which was not diſcovered till they were diſſolved in the acid, and which makes the experiment leſs deciſive than it would otherwiſe be. The quantity of fixed air obtained is alſo much greater than, according to Mr. BERGMAN's experiment, could be yielded by the plumbago uſually contained in 1000 grains of iron; ſo that though the experiment ſeems to ſhew that the fixed air was only produced by the decompoſition of the impurities in the filings, yet it certainly ought to be repeated in a more accurate manner.

Before I conclude this paper, it may be proper to ſum up the ſtate of the argument on this ſubject. There are five methods of phlogiſtication conſidered by me in my paper on air;

namely, firſt, the calcination of metals, either by themſelves or when amalgamated with quickſilver; ſecondly, the burning of ſulphur or phoſphorus; thirdly, the mixture of nitrous air; fourthly, the exploſion of inflammable air; and, fifthly, the electric ſpark; and Mr. KIRWAN has not pointed out any other which he conſiders as unexceptionable. Now the laſt of theſe I by no means conſider as unexceptionable, as it ſeems much moſt likely, that the phlogiſtication of the air in that experiment is owing to the burning or calcination of ſome ſubſtance contained in the apparatus *. It is true, that I have no proof of it; but there is ſo much probability in the opinion, that till it is proved to be erroneous, no concluſion can be drawn from ſuch experiments in favour of the generation of fixed air. As to the firſt method, or the calcination of metals, there is not the leaſt proof that any fixed air is generated, though we certainly have no direct proof of the contrary; nor did I in my paper inſinuate that we had. The ſame thing may be ſaid of the burning of ſulphur and phoſphorus. As to the mixture of nitrous air, and the combuſtion of inflammable air, it is proved, that if any fixed air is generated, it is ſo ſmall as to elude the niceſt teſt we have. It is certain too, that if it had been ſo much as $\frac{1}{70}$th of the bulk of the common air employed, it would have been perceived in the firſt of theſe methods, and would have been ſenſible in the ſecond though ſtill leſs. So that out of the five methods enumerated, it has been ſhewn, that in two no ſenſible quantity is generated, and not the leaſt proof has been aſſigned that any is in two of the others;

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* In the experiment with the litmus I attribute the fixed air to the burning of the litmus, not decompoſition, as Mr. KIRWAN repreſents it, which is a ſufficient reaſon why no fixed air ſhould be found when the experiment is tried with air in which bodies will not burn.

others; and as to the laſt, good reaſons have been aſſigned for thinking it inconcluſive; and therefore the concluſion drawn by me in the above-mentioned paper ſeems ſufficiently juſtified; namely, that though it is not impoſſible that fixed air may be generated in ſome chemical proceſſes, yet it ſeems certain, that it is not the general effect of phlogiſticating air, and that the diminution of common air by phlogiſtication is by no means owing to the generation or ſeparation of fixed air from it.